REVIEW OF 'THERMAL STABILITY OF A DC/DC CONVERTER WITH INDUCTOR IN PARTIAL SATURATION'

TRAVIS C. MALLETT

ABSTRACT. This is a post-publication review of the article "Thermal Stability of a DC/DC Converter with Inductor in Partial Saturation" published in *IEEE Transactions on Industrial Electronics.* The review summarizes the article and explains its overall strengths and impact. The analysis, simulations, and test results are straightforward and show a satisfying match between the proposed model and test results. However, the authors should carefully consider the assumptions in their model and better justify any simplifications. This review requests an extension of the thermal stability analysis section. Results should include a comparison with other modeling methods, and the results should also compare the simulation with measurements during an unstable transient. Finally, the review asks the authors to better clarify the novelty of their contribution.

1. Overall summary of the article and its findings

This paper [1] develops a model of a DC/DC converter which includes a thermal model of an inductor in partial saturation. The model is compared with experimental results. Thermal stability of the converter and inductor is discussed.

2. Overall strengths of the article and its impact in the field

The aim of the paper is clear and concise and clearly described by the abstract and title. Thermal stability is a clear issue in DC/DC converters, especially as size constraints on inductors become more problematic. This article is relevant to the industry. Overall, the article is clear and well-written. After some major revisions, this paper can be accepted for publication in IEEE Transactions on Industrial Electronics.

3. Major points which need clarification, refinement, reanalysis, and rewrites

The analysis, simulations, and test results are straightforward and show a satisfying match between the proposed model and test results. However, the authors should carefully consider the assumptions in their model and better justify any simplifications. The thermal stability analysis section should be extended. Results should include a comparison with other modeling methods,

T. C. Mallett is with Schweitzer Engineering Laboratories, Inc. in Pullman, WA. This review was published by Publons on August 12$^{\text{th}}$, 2020. DOI: 10.14322/PUBLONS.R8935321.

and the results should also compare the simulation with measurements during an unstable transient. Finally, the authors should better clarify the novelty of their contribution.

3.1. **Comments on Introduction.**

(1) The introduction does a good job of describing the problem: positive feedback between the inductor temperature and peak current (due to change in inductance) can cause current to increase "until a thermal collapse occurs." The introduction then describes many papers which deal with the simulation of thermal transients in DC/DC converters. However, the introduction does not describe a further problem statement such as "previous attempts are inadequate because. . . " or "however, these methods do not consider magnetic losses which is critical to attaining an accurate model." Please provide a problem statement which justifies the need for this research considering the previous research.

(2) In attempting to clarify the novelty of their paper, the authors suggest that their approach "differs from [5, 13] and from [8] since it proposes a model of the inductor based on a polynomial curve; this approach is similar to [20] . . . " But earlier in the introduction section, the authors state that "the inductor is modeled by a polynomial curve as in [17]." Later, in Section IV.C, the authors cite "[19, 23]" in reference to modeling the inductance in partial saturation with a polynomial function. Thus, on the one hand, the authors appear to be advertising the polynomial model as part of the novelty of their method (see Introduction section: "The novelty consists in [. . .] a model of the inductor based on a polynomial curve . . . ") while their article provides much evidence and many citations indicating there is nothing novel about this idea. The only novelty remaining, then, is their statement that "this approach is similar to [20], but here it is improved taking into account Ohmic and magnetic losses." In short, the novelty is in a very confused state, and the authors should greatly clarify the novelty of their contribution. As-is, I am skeptical about the scientific value this article brings because every detail of this model has been investigated in depth before. By simply combining standard equations and losses in the model, the authors are not doing much more than an engineering exercise.

(3) Additionally, thermal stability investigations are common practice in industrial electronics development, and inductors in power supply applications are common targets of such analysis. Although the model is generally nicely done and would be acceptable as part of a design document in the industry, the authors have not really presented a convincing case of the novelty of their ideas and contribution to academia.

3.2. **Comments on Methodology.**

(1) The test was performed on a Panasonic ELC18B221L "since it is a widely used inductor." Unfortunately, this inductor was obsoleted by the manufacturer on September 25, 2019 [2]. This doesn't negate any of the results, but it does negate the reason for selecting this component. The authors should either re-test on another inductor in a mature life cycle or remove or reword the reason for selection.

(2) Equation (2) gives the "correction coefficient K_T whose expression is given by [21]." However, I cannot find this equation anywhere in [21]. Reference [21 (# in author's paper)] has no mention of skin effect. If Equation (2) is somehow derived from material in [21 (# in author's paper)], it should be made clear. Otherwise, the correct reference must be given.

(3) The authors account for Ohmic losses in (1). But this is a simplified formula. The resistivity of copper wire also depends on temperature. It is not clear why this is neglected in the model. The authors do mention that "R_J increases with a further increase of Joule losses that depend on the square of the current" (Section IV.B). But as shown in Fig. 3, the Joule losses in the wire only contribute to the P_{LL} and do not feedback into the calculation of R_J. Furthermore, that feedback will have its own thermal time constant. The authors should analyze whether this simplification is justified and include a proper justification in the article.

(4) The block diagram in Fig. 3 shows the Joule losses from R_J adding directly to the P_{LL}, but in practice, this is not the case. There is a complex thermal coupling between R_J joule losses and the contribution to the *effective* P_{LL} that contributes to the temperature of the core. I.e., it is not expected that the Joule losses and the magnetic losses will both contribute to the thermal model input in the exact same way. The authors do show some semblance of this type of feedback in Fig. 3: the current $I_{LL}(t)$ feeds into the "Equivalent resistance calculation" which produces I_{eq_m} which contributes back to $I_{LL}(t)$ (and then back into R_J). But this only shows the effect of the magnetic losses modeled by equivalent resistance. The authors should analyze whether the simplification of no feedback directly into R_J is justified and include a proper justification in the article.

(5) Based on the above questions of inadequately justified simplifications, I wonder about the validity of equations (18) and (19). Including the above thermal interactions in the model means that the transfer function in (18) is certainly not first-order. I thus also question (19) which is such a simplified condition for stability that its usefulness is doubted. The conclusion on the stability hinges on whether ζ is increased due to environmental factors. This may be a dangerous conclusion to draw since additional factors may be important. The authors should expand the thermal analysis to include more interactions. Then the Thermal Stability Evaluation in section V can draw more insightful conclusions.

Currently, section V is so simplified as to have almost no useful information. I expected the conclusions regarding thermal stability to be the main thrust of the paper based on the abstract and introduction and found the resulting analysis disappointing.

(6) The real question that the authors set out to understand is the thermal stability of the *converter* (see abstract and introduction sections: "...can jeopardize the thermal stability of the converter" and "analyze the thermal stability of a DC/DC converter," respectively). Yet the thermal stability analysis (section V) only applies to the inductor. The authors then present a model of the converter in Simulink and then move onto the experimental and simulation results. That is, while the authors may have created an effective model of a DC/DC converter, they appear to skip over analytical evaluation of the stability of the converter itself and then the rest of the article compares the model (simulation) against the test results. The stability of the converter is mostly shown from the improved model by way of the example in Fig. 15. But this is only an example that pertains to the author's specific circuit and inductor. The scientific value of this paper would be dramatically increased if more time is spent analyzing the stability of the converter (rather than comparing the model to the simulation). Currently, the paper advertises an analysis of thermal stability but ends up with just another model of a DC/DC converter that includes thermal characteristics.

(7) The thermal stability analysis also does not account for component deviations. These can, of course be simulated by means of a Monte Carlo analysis, or similar. But a robust stability analysis would be a great contribution to the literature. For example, the important parts of the model may be analyzed using Kharitonov's Theorem showing robust stability over a range of parameter values [3]. This would be an especially important contribution to industrial electronics which, in practice, cannot rely on the assumption that a stability analysis based on a single inductor's characterization will be sufficient for high-reliability applications.

3.3. **Comments on Results.** The results are presented clearly and are understandable. Thank you for including comparison tables for the output voltages in Tables II and III. Please address the points below.

(1) The authors do not present any comparisons with other models. The novelty of their contribution is thus again questioned. Please also include simulation results for a more simplified model (e.g., short out the "thermal model" in Fig. 3). This will allow the accuracy improvement of the model to be clearly displayed.

(2) The authors compare simulation and experimental data for several stable cases. But the unstable transient in Fig. 15 is only calculated. It would be very interesting for the authors to induce an unstable transient in the converter and compare with the simulation, even if

the authors need to modify their converter to be more unstable to make the measurement easier to obtain. It would be great to see the model match experimental results in an unstable case. The photo of the damaged inductor in Fig. 16 makes me think the authors did, in fact, try this. But I question why results are not included? Even if the model fails to precisely follow the unstable transient in Fig. 15, this would be good information to know.

3.4. Comments on Discussion and Conclusion.

(1) Once the previous feedback is incorporated, the conclusion section can be made to include more specificity as to the results (e.g., include a % improvement in the model accuracy over previous models). Additional insights into the importance of various parameters on thermal stability may also be included in the discussion based on the extended thermal analysis requested in this review. For example, the conclusion may state "the thermal time constant of the inductor was found to be the most important parameter affecting the thermal stability of the DC/DC converter."

(2) The authors did not provide any direction for future extensions to their work. Please include some guidance for enthusiastic readers by providing future research directions on the topics under investigation.

4. Minor points about figures/tables, references, typos, and other inconsistencies

(1) The use of "anyway" on page 3 is too informal.

(2) The authors should also include the following keywords in the Keywords section: Thermal stability, Robust stability, Thermal analysis

References

[1] G. Vitale, G. Lullo and D. Scire, "Thermal Stability of a DC/DC Converter with Inductor in Partial Saturation," *IEEE Transactions on Industrial Electronics,* Bangkok, 2018, pp. 1135-1139, doi: 10.1109/TIE.2020.3014580.

[2] Panasonic, Product Discontinuation Notice: ELC and ELL Series Choke Coil, 2019, Accessed 14 August 2020, SiliconExpert.com.

[3] Bhattacharyya, Shankar P., and Lee H. Keel, Robust control: the parametric approach. Pergamon, 1995